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Ueber Dr. Wichmann's Bestimmung der Parallaxe des Argelander'schens Sterns, von W. Döllen. (Bulletin Physico-Mathétique, tome xiii.) St. Pétersbourg, 1854.

This paper is devoted to a critical examination of Dr. Wichmann's researches on the parallax of Argelander's star (1830 *Groombridge*). The author states that it was placed in the hands of M. W. Struve as early as the month of June 1853, although, for a reason explained by him, its publication was delayed for some time.

After a few preliminary remarks, the author commences by giving a concise statement of the nature of the inquiry undertaken by Dr. Wichmann, and of the conclusions at which he arrived. The observer measures the distance between two stars, p and s, separate from each other by an interval of about 3200″. The results obtained by measures of this kind exhibit variations of a periodic character, and these being discussed upon the supposition of their being due to parallax, it has been found that the difference of parallax of the two stars amounts to 1″·17 with a probable error of 0″·08. Again, the observer measures the distance of the two stars p and s from a third star A (Argelander's star), situate nearly midway between them; and the periodic variations in the *differences* of these distances, which are very nearly equal in quantity and direction, lead to the conclusion that the excess of the parallax of A over half the sum of the parallaxes of p and s amounts to 0″·135, with a probable error of 0″·013.

The author, in his examination of the reasoning by which these results were deduced, has recourse, first, to considerations of a general nature, and then endeavours to substantiate his views by a process of calculation. He commences with an allusion to the many fruitless attempts which have been made to ascertain the parallax of the fixed stars. The memoir of Dr. Peters on this subject affords a highly instructive picture of the persevering efforts of the human mind in search of truth. These efforts, although unattended with success, in so far as the object of pursuit was concerned, have, notwithstanding, proved very advantageous to Astronomy, from the valuable improvements of the methods of investigation which they have suggested, and the astonishing perfection which they have occasioned in the fabrication of instruments of observation.

In Stellar Astronomy the progress of improvement has been such, that the objects of investigation may now be considered to be of the same order of magnitude as the disturbances by which the condition of the instrument in each individual instance of observation is inevitably affected. The peculiar feature of modern observation consists in the circumstance that the principal difficulties which offer themselves to the observer, reside not in the heavens, but in the instrument with which his observations are made. These must be overcome before any ulterior success in his inquiries can be expected.

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To the astronomer the most perfect instrument is not that which is affected by the smallest errors, but that whose errors he is best acquainted with. It is not sufficient, however, that the observer has been enabled, *in any given instance*, to determine the errors of his instrument. In order to render his observations available for theoretical purposes it is desirable that he should know how to eliminate the errors by which his instrument is affected on each individual occasion of observation. This being generally impracticable, it only remains to make as little use as possible of the supposition of the invariability of the instrument. A clear perception of the necessity for adopting this course has led to the two most extensively-employed principles of modern observation, namely, the principle of the immediate reversal of the instrument, and the principle of the simultaneous measurement of the differences of two nearly equal quantities affected by similar circumstances. It must be acknowledged that the latter of these is in practice preferable to the former. In fact, the instrument *after* reversal, and even by virtue of that operation, is no longer in the same condition as that in which it previously was ; and, notwithstanding that we have thereby eliminated certain sources of error, still it is to be feared that we have at the same time introduced new errors of an unknown character. It is true, indeed, that in the present state of observation an advantage is gained by the process of reversal ; but it is possible that the day may come when this will no longer be so.

No such contingency can, however, occur in the case of the differential method of observation, the advantages of which become more obvious as the observations are more accurate, and as the quantities to be measured are smaller. And yet it is important to remark that, from the nature of this method, the conditions which assure its superiority are not rigorously fulfilled. In each particular case the quantities to be measured are not precisely equal ; they are not affected by precisely the same circumstances, nor are they observed at precisely the same times. It is manifest, therefore, that there exists a determinate limit beyond which the advantages of the method cease to be appreciable. Hence arises the desirableness of obtaining some test by which we might be enabled in any instance to ascertain whether this limit has been attained. This object is frequently effected by a comparison of the probable errors of the same result, as determined by different combinations of the observations. Should any discordance manifest itself in the values of the probable errors as thus deduced, it is thereby demonstrated that the errors of the observations are not merely accidental, but, to a certain extent, regulated by a fixed law. Without knowing this law, it is so far instructive to know that by the combination which assigns the least probable error, the unknown errors of the observations are most effectually diminished. By this means we may generally rely upon having rendered the errors wholly or partially innocuous, although we are unable to ascertain either their nature or their magnitude.

The author contends at great length against the views of Dr.

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Wichmann relative to the existence of a sensible parallax of the star *p*. He is of opinion that such a result is of itself extremely improbable, and he adduces various considerations in support of his assertion. He remarks, for instance, that if the star had the parallax which has been assigned to it by Dr. Wichmann, it ought to have a sensible proper motion. The motion of the solar system in space, which is no longer merely an hypothesis, being directed to a point in the heavens which is distant 77° from the star, almost the whole effect of such a motion ought to exhibit itself in an apparent displacement of the star, to an extent depending on the largeness of the star's parallax. But, since the star has been found to have no sensible apparent motion, it follows that it must have a real motion, the projection of which upon the celestial sphere is equal in magnitude, but opposite in direction, to the effect produced by the motion of the solar system in space. "Now such a coincidence," says the author, "is naturally not impossible, and we should certainly not be warranted in maintaining its improbability in opposition to a result deduced from observation, if we had no reason to doubt the truth of that result. But, on the other hand, to declare that an hypothesis which leads to such a conclusion, is one of a plausible character would be equally untenable."

In the second part of his memoir the author proceeds to consider the disturbing causes which he conceives may have affected the heliometric observations forming the groundwork of Dr. Wichmann's investigation. These he refers to the influences of temperature and flexure. With respect to the former, he is of opinion that the value of the thermometer coefficient as found by Bessel is entitled to reliance as a normal determination of the element; but that the case is quite different when the question refers to any specific measurement. The value of a revolution of the screw, which requires to be introduced into the calculations, is a function of the temperature, not merely of the screw, but also of the material constituting the object-glass; and these two arguments always operate in contrary directions. If the temperature rises, the screw extends, and the value of a revolution is thereby *increased;* on the other hand, an increase of temperature produces an extension of the focal length of the object-glass, and hence arises a *diminution* in the value of a revolution of the screw. According to Bessel the effect produced by those two opposing causes is in favour of the latter; in other words, an *increase* of temperature will tend on the whole to *diminish* the value of a revolution of the screw. The author remarks that the partial compensation which takes place under such circumstances had suggested to him certain ideas on the subject which he does not remember to have found elsewhere. Should these individual effects be nearly equal, and, what is quite possible, considerably greater in each case than their difference, then, in the application of the temperature correction, an error in the thermometric quantity employed as the groundwork of calculation is of much less consequence than an error in the tacitly-assumed supposition that the screw and the object-glass have both the *same* temperature. Now this supposition is rarely, if ever, realised; and it is doubtless exceedingly difficult to ascertain the exact amount of difference in any individual case. The author describes an experiment illustrative of the different effects produced by exposing to the same temperature two thermometers, having bulbs of *unequal* magnitudes. He contends that a similar effect must be produced upon the screw and the object-glass of the heliometer, which are so dissimilar in form and magnitude, and the materials of which possess such unequal powers of conducting heat. He remarks further, that we ought to take into consideration not only the difference of temperature of the *different parts* of the instrument, but also the change of temperature which occurs *during the time of observation.* The adjustment of the eye-piece, with respect to the object-glass, which depends upon the temperature of the latter, is effected before the commencement of each set of observations. On the other hand, the temperature of the screw may be considered as coincident with the indications of the thermometer corresponding to the time of each individual observation. Supposing each set of observations to occupy an hour, the reading of the thermometer corresponding to the middle of this interval may be taken for determining the mean temperature of the screw. The question then arises, how high shall we estimate the difference between the temperature of the screw at a given instant, and the the temperature which determines the focal length of the object-glass half-an-hour earlier. Supposing this to amount to $\pm 10^\circ$ Fahrenheit, the author finds that the correction to the temperature coefficient hence arising may amount to $\pm 0''\cdot 3$, a quantity which he shows to be of the same order as the principal part. He maintains, therefore, that he was justified in asserting that the question with respect to the influence of temperature upon the instrument cannot be considered as exhausted.

From the question of temperature, the author passes to a consideration of the influence of flexure. After a full discussion of this subject he takes up a series of the observations contained in Dr. Wichmann's paper, and forms, by means of them, a set of equations of condition which he solves upon various suppositions with respect to temperature, flexure, and parallax. The final result of these various essays may be best stated in his own words. "It has been demonstrated," says he, "that the observations under consideration cannot be satisfied by any possible value of the temperature co-efficient, nor by my formula for flexure, nor by parallax, nor by all together; other disturbing influences, regulated by a fixed law, must be in operation. The existence of such disturbing influences being once established, it is certainly not admissible to attach to any determination deduced from these observations a degree of confidence proportionate to the probable error obtained by means of them. In fact, upon what grounds could

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we expect that these unknown disturbances had operated exclusively in the direction of proper motion, and not equally in the direction of parallax, or flexure, &c. For my own part, I am of opinion that to attempt to solve the enigma by the assumption of any other law of flexure, or by a modification of the correction for temperature, were an undertaking which, from the foregoing details, would seem to afford no prospect of success. I must stop here; I must content myself with having shown by this example what I had already established by considerations of a general nature, that in the present condition of our knowledge respecting the influence which an instrument such as the heliometer exercises upon the results of observation, it were utterly impossible to arrive at a reliable conclusion with respect to the cause of the periodic variations amounting to one second of space which the measures of a distance extending to about 59 revolutions of the screw, or 52′, exhibit in the course of a year. The undeniable presence of such periodic variations is, however, a fact of such weighty importance that we must not endanger our possession of it by a hasty decision. I here repeat what I have so often and so emphatically stated already, that only a renewed investigation of the subject, carefully prosecuted with reference to the special circumstances of each case under consideration, can lead to a definite result; but we may expect by this means to arrive at a conclusion of a trustworthy nature."

The author proceeds to remark that his criticisms must not be considered as implying that the heliometer is incapable of furnishing any essential contributions to our knowledge of the parallax of the fixed stars. On the contrary, he is of opinion that, in consequence of the large scope which it allows for selecting the stars of comparison, and the extraordinary accuracy with which its adjustment can be specially effected, it is peculiarly adapted for such inquiries. He then shows that, by combining together the results of Schlüter and Wichmann relative to the parallax of A, a value of the latter will be obtained, which is almost entirely free from the error of the instrument, *according to whatever law depending on the distance we may imagine it to operate.* In this way he finds the parallax of the star to be 0″·141, with a probable error of 0″·013, supposing the parallaxes of the stars of comparison to be evanescent,—an assumption which, in the present state of our knowledge, he considers to be perfectly justifiable. He remarks that the value of the parallax deduced from a suitable comparison of this result with the corresponding results of Peters and Otto Struve which he also considers to be worthy of confidence, must be considered as one of the most trustworthy determinations connected with the subject; and although it may not chime very accordantly with our notions in the present day respecting the distances and real proper motions of the stars, this circumstance merely indicates to us that we have still much to rectify in respect to those notions.

The author concludes his paper with a vindication of M. Otto